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Are there Political Budget Cycles in the Euro Area?

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ABSTRACT

This article examines whether there is a political budget cycle (PBC) in countries in the euro area. Using a multivariate model for 1999–2004 and various election indicators we find strong evidence that, since the start of the Stability and Growth Pact, fiscal policy-makers in the euro area have pursued expansionary policies before elections. In an election year – but not in the year prior to the election – the budget deficit increases. This result is in line with third-generation PBC models, which are based on moral hazard. We also find a significant but small partisan effect on fiscal policy outcomes.

KEY WORDS

- fiscal policy
- political budget cycle
- Stability and Growth Pact

It is often thought that incumbents try to use expansionary fiscal policy before elections to increase their popularity. However, most empirical studies examining the existence of a political budget cycle (PBC)[1] in industrial countries find little support for such election-driven fiscal policy manipulations.[2] Some of these studies examine whether a PBC exists in member countries of the European Union (EU). For example, Andrikopoulos et al. (2004) investigate whether incumbent governments manipulated fiscal policy in order to enhance their re-election prospects. Their empirical evidence for the 1970–98 period does not support this hypothesis. De Haan and Sturm (1994), who examine fiscal policy in the European Community during the 1980s, also find no support for PBCs. However, other studies focusing on the 'old' member countries of the EU claim that in more recent years fiscal policy has often turned expansionary before elections. For instance, Buti and Van den Noord (2003) report electoral effects based on an examination of the discretionary part of fiscal policy over the 1999–2002 period. Also Von Hagen (2003) concludes that there is evidence for a PBC in EU member countries. For the period 1998–2002 he finds that the (unweighted) average fiscal impulse in pre-election years was significantly higher than the average fiscal impulse in all other country-year cases.[3]

The results of these last two papers are somewhat surprising since member countries of the EU are restricted in their fiscal policies by the Stability and Growth Pact (SGP). The SGP consists of two Council Regulations: one on the strengthening of the surveillance and coordination of budgetary positions, and the other on speeding up and clarifying the implementation of the excessive deficit procedure. A corresponding European Council Resolution ties them together. Regulation 1466/97 sets out to strengthen multilateral surveillance and gives member states a goal of a medium-term budgetary position of close to balance or in surplus. Regulation 1467/97 clarifies and accelerates the excessive deficit procedure as specified in the Maastricht Treaty so that – in principle – within 10 months non-interest-bearing deposits and ultimately fines can be imposed if the member state concerned takes no effective actions to redress so-called excessive deficits (measured against reference values of 3% of GDP for the general government budget deficit and 60% for the general government debt-to-GDP ratio).[4] According to Buti and Van den Noord (2003: 4), the SGP 'is unquestionably the most stringent supranational "commitment technology" ever adopted by sovereign governments on a voluntary basis in the attempt to establish and maintain sound public finances. The SGP, if applied according to its letter and spirit, will have important implications for the behaviour of budgetary authorities in both the short term (cyclical stabilization, policy co-ordination) and long term (sustainability of public finances).'

This paper examines whether there really exists a PBC in EU member countries in the euro area. We focus on EU members that have adopted the euro because the rules of the SGP are less strict for member countries that are not part of the currency union. In contrast to Buti and Van den Noord (2003) and Von Hagen (2003), we use a multivariate model. Furthermore, instead of employing election dummies – like most previous PBC studies – we use an election variable suggested by Franzese (2000) that takes the timing of an election in the course of a year into account. On the basis of data for the period 1999–2004, we find strong evidence that the SGP has not restricted fiscal policy-makers in the euro area in pursuing expansionary policies before elections. In an election year – but not in the year prior to the election – the budget deficit increases. This result is in line with third-generation PBC models, which are based on moral hazard. We also find that the political composition of the government has a significant (but small) influence on net lending.

The remainder of the paper is organized as follows. After offering some theoretical considerations and reviewing previous empirical studies, we outline the empirical model. The fourth section contains the main results and the fifth section presents the outcomes of various sensitivity tests. The final section offers some concluding comments.

Theoretical considerations and previous empirical studies

Three generations of theoretical PBC models can be distinguished.[5] The first-generation models emphasize the incumbent government's intention to secure re-election by maximizing its expected vote share at the next election (Nordhaus, 1975). It is assumed that the electorate is backward looking and evaluates the government on the basis of its past track record. As a result, these models imply that governments, regardless of ideological orientation, adopt expansionary fiscal policies in the late year(s) of their term in office in order to stimulate the economy.

The second-generation models – Shi and Svensson (2004) call them adverse-selection-type models – emphasize the role of temporary information asymmetries regarding the politicians' competence level in explaining electoral cycles in fiscal policy. In these models, signalling is the driving force behind the PBC. The first of these models is due to Rogoff and Sibert (1988), who assume that each political candidate has a competence level (high or low) that is known only to the politician and not to the electorate. Voters want to elect the more competent politician and form rational expectations regarding the incumbent's type based on observable current fiscal policy outcomes. Before the election, high-type incumbents will attempt to signal

their type (and thereby increase their chances of re-election) by engaging in expansionary fiscal policy, which is less 'costly' for them than it is for low-type incumbents. This leads to a pre-election increase in the government deficit when competent politicians are in office. Rogoff (1990) argues that incumbents can also signal their competence before an election by shifting government expenditure towards easily observed consumption spending and away from investment. As pointed out by Shi and Svensson (2004), some of the implications of the signalling models seem to be at odds with both empirical and anecdotal evidence. For example, only more competent politicians (rather than less competent ones) distort the economy in the separating equilibrium of the signalling game. Likewise, only competent politicians will be re-elected. Furthermore, since only competent types signal by creating a boom before an election, the testable implications are unclear without additional information on the (unobservable) type of the incumbent.

These drawbacks do not apply to the third-generation PBC models, which are based on moral hazard. Examples of such studies are Persson and Tabellini (2000) and Shi and Svensson (2002). As in the adverse-selection models, each politician has some competence level that is unknown to the electorate. Additionally, it is assumed that politicians cannot observe their competence level ex ante either. That is, politicians are uncertain about how well they will be able to handle future problems. Voters are rational and therefore want to elect the most competent politicians because that would imply higher levels of post-election public goods production. The constituents' inference is based on the observable macroeconomic performance of the incumbent government. The key assumption is that the incumbent government can exert a hidden effort, that is, use a policy instrument unobservable to the public that is a substitute for competence. For example, if competence measures how well politicians can convert revenues into public goods, the hidden effort can be interpreted as the government's short-term excess borrowing. Elections take place after the incumbent government's hidden effort and competence have jointly determined the observable macroeconomic outcome. The incumbent government would like to increase its performance index by exerting more effort (borrowing more), hoping that voters will attribute the boost in public goods provision to its competence. In the equilibrium of this moral hazard game, there will be an excessive effort on the part of the incumbent politicians, and, as a result, there is an increase in the budget deficit prior to an election. In these models, all types of incumbent government will incur excessive pre-election budget deficits (independent of their competence level). As Shi and Svensson (2004) point out, one can test these empirical predictions whether or not the type of the incumbent government is observable. Furthermore, the implications of this model can be tested for all democratic countries, irrespective of their political structure (e.g. the existence of partisan differences).

Brender and Drazen (2005) argue that until recently a PBC was generally thought to be a phenomenon of less developed economies. For instance, Schuknecht (1996) reports evidence for a PBC in his sample of 35 developing countries over the period 1970–92 and Block (2002) finds for a cross-section of 44 sub-Saharan African countries that the government fiscal deficit increases by 1.2 percentage points in election years. Likewise, Schuknecht (2000) finds for a sample of 24 developing countries for the 1973–92 period that incumbent governments tend to increase public investment prior to elections. Hallerberg et al. (2002) consider whether political business cycles existed in East European accession countries during the period 1990–9. They find that governments of accession countries with a fixed exchange rate regime manipulate their economy in election years through running larger budgets.

However, some recent studies present evidence for the existence of a PBC in a large cross-section of both developed and developing countries. For instance, Shi and Svensson (2002) show that significant pre-electoral increases in the government budget deficit exist for their panel of 91 developing and developed countries over the period 1975–95. Moreover, Persson and Tabellini (2002) report statistically significant tax decreases before elections in a sample of 60 democracies over the period 1960–98. Brender and Drazen (2005) suggest, however, that the results of these studies are driven by the experience of so-called 'new democracies', where fiscal manipulation may be effective because of a lack of experience with electoral politics in these countries. They argue that, once the 'new democracies' are removed from the sample, the PBC disappears. Nonetheless, some recent studies focusing on 'established democracies' find evidence for the existence of a PBC. For instance, Alt and Lassen (2006) argue that among advanced democracies significant opportunistic electoral cycles are conditional on the transparency of budget institutions. In countries with less transparent institutions the electoral cycle in fiscal policy appears, whereas no such election-related fiscal policy movements show up in higher-transparency countries. However, Tujula and Wolswijk (2004) find support for a PBC independently of fiscal institutions in their sample of OECD countries for the period 1970–2002. Likewise, Buti and Van den Noord (2003) and Von Hagen (2003) claim that fiscal policy in EU member countries turns expansionary before elections despite the introduction of the SGP. In the following sections we will examine whether there are indeed political budget cycles in the euro area.

Empirical model and data

In the PBC literature, several indicators of fiscal policy have been suggested. The three measures most often used are net lending, cyclically adjusted net

lending, and the primary budget balance. As part of the sensitivity analysis presented below we have employed all these measures, but in reporting our main findings we primarily focus on net lending. The cyclically adjusted budget balance suffers from the fact that there is no consensus on how it should be computed; figures provided by various international organizations often show large differences. Furthermore, De Haan et al. (2004) demonstrate that OECD figures on cyclically adjusted balances are subject to large revisions for a rather long time after their initial publication. Because we are focusing on a very recent time period, we therefore prefer to use unadjusted figures. The primary balance has the advantage that it excludes interest payments, which are not controlled by the government. However, although interest payments are more or less exogenous, policy-makers can cut other expenditures if interest payments rise; a reluctance to do so during an election year should certainly be considered relevant from a PBC perspective.

Some indicative evidence for the existence of a PBC in the euro area is shown in Table 1, which presents an overview of the budget balances for all countries in the currency union for the period 1999–2004. Figures for the 20 election years are in bold. It immediately becomes clear that all countries, with the exception of Portugal, had on average lower budget balances during election years than during years without a parliamentary election. The (unweighted) average budget balance for all election years in the sample equals –1.4%, whereas for election years it is –0.5%. The *t*-statistic for the difference between the two values is 1.42, which is significant at the 10% level. However, in order to draw more compelling conclusions regarding PBCs in the euro area, a model is required that takes into account other potentially relevant determinants of the budget balance.

Apart from electoral influences, our empirical model accounts for two factors that are not directly controlled by policy-makers but that affect fiscal policy outcomes. First, to filter automatic stabilization effects from our fiscal policy measure, we include the output gap as a proxy for the business cycle. An economic upswing will lead to an increase in the budget balance, whereas in the downward phase of the business cycle the balance usually deteriorates. Because there is no consensus in the literature on how to construct the output gap, we use output gaps based on potential GDP as well as on trend GDP, both calculated by the European Commission.

Second, since unforeseen economic developments may affect fiscal policy outcomes, we include the difference between actual and expected real GDP growth. The expected growth rates are the GDP growth forecasts available at the time the government budget was decided upon. A higher than expected growth rate will lead to an increase in the budget balance owing to, amongst other things, unexpected increases in tax receipts. Likewise, unexpected inflation may have an effect on the budget balance. As pointed out by Tujula

Table 1 Net lending (% GDP) in EMU member states: 1999–2004

	1999	*2000*	*2001*	*2002*	*2003*	*2004*	*Election years*	*Non-election years*	*All years*
Austria	**–2.3**	–1.5	0.3	**–0.2**	–1.1	–1.3	–1.3	–0.9	–1.0
Belgium	**–0.4**	0.2	0.6	0.1	**0.4**	0.1	0.0	0.3	0.2
Finland	**2.2**	7.1	5.2	4.3	**2.5**	2.1	2.4	4.7	3.9
France	–1.8	–1.4	–1.5	**–3.2**	–4.2	–3.7	–3.2	–2.5	–2.6
Germany	–1.5	1.3	–2.8	**–3.7**	–3.8	–3.7	–3.7	–2.1	–2.4
Greece	–3.4	**–4.1**	–3.6	–4.1	–5.2	**–6.1**	–5.1	–4.1	–4.4
Ireland	2.6	4.4	0.9	**–0.4**	0.2	1.3	–0.4	1.9	1.5
Italy	–1.7	–0.6	**–3.0**	–2.6	–2.9	–3.0	–3.0	–2.2	–2.3
Luxembourg	**3.4**	6.2	6.2	2.3	0.5	**–1.1**	1.2	3.8	2.9
Netherlands	0.7	2.2	–0.1	**–1.9**	**–3.2**	–2.5	–2.6	0.1	–0.8
Portugal	**–2.8**	–2.8	–4.4	**–2.7**	–2.9	–2.9	–2.8	–3.3	–3.1
Spain	–1.2	**–0.9**	–0.5	–0.3	0.3	–0.3	–0.6	–0.4	–0.5
Average (unweighted)	–0.5	0.8	–0.2	–1.0	–1.6	–1.8	–1.4	–0.5	–0.7

Sources: see appendix on the EUP webpage.

and Wolswijk (2004), inflation may affect government receipts and expenditures through nominal progression in tax rates and tax brackets, and via price indexation of receipts and expenditures. On the other hand, unexpected inflation erodes the real value of nominal government debt so that the overall effect of inflation on the budget balance is not clear a priori.

After accounting for the effects of business cycle fluctuations and forecasting errors in GDP growth and inflation, all remaining changes in the budget balance are considered to be the result of discretionary fiscal policy. These considerations suggest estimating the following equation:

$$\begin{aligned}\Delta BB_{it} &= \beta_0 + \beta_1\left(y_{it} - y_{it}^{f}\right) + \beta_2\left(\pi_{it} - \pi_{it}^{f}\right), \\ &+\beta_3\left(\Delta GAP_{it}\right) + \beta_4(\textit{Election Measure}) + \varepsilon_{it}\end{aligned} \tag{1}$$

where ΔBB_{it} is the change in country is overall budget balance as measured by the change in net lending; $y_{it} - y_{it}^{f}$ stands for the difference between actual real GDP growth at time t and real growth as forecast at the time fiscal policy for year t was determined; $\pi_{it} - \pi_{it}^{f}$ represents the difference between actual inflation at time t and inflation as forecast at the time fiscal policy for year t was determined; ΔGAP_{it} measures the change in the output gap in year t as a percentage of GDP.

In order to take account of potential election effects, two alternative indicators are considered. The first involves adding a dummy to the equation that indicates whether a year is an election year or not ($D^{\text{Election year}}$). A similar dummy is used to account for pre-election years. Because fiscal policy decisions may take some time to have an effect, election-induced fiscal measures could be taken in the year prior to the actual election.

The second indicator takes the timing of an election in the course of the year into account. It has been developed by Franzese (2000) and is calculated as:

$$ELE = [(M - 1) + d/D]/12, \tag{2}$$

where M is the month of the election, d is the day of the election and D is the number of days in that month. For the year prior to the election year, the indicator is calculated as:

$$ELE = [12 - (M - 1) - d/D]/12. \tag{3}$$

In all other years its value is set to zero. For reasons that will become clear later, we want to be able to interpret the values of the election indicator in terms of actual election dates. This means that an indicator value of, for instance, 7/12 in an election year as well as a value of 5/12 in a pre-election year implies that elections take place at the end of July. In general, this requires that snap elections are ignored because the length of the period during which *election-driven* policy manipulations can occur – that is, the period between a government crisis and the early election – does not say anything at all about the actual election date because it usually does not amount to exactly 365 days. As a result of these considerations, 19 observations for election years and 12 observations for pre-election years can be used in calculating the election indicator.

The macroeconomic data necessary to estimate the model have been obtained from stability programmes of the EU countries and official publications of the European Commission. Data on general election dates are mainly taken from political data yearbooks (Katz and Koole, 1999, 2000, 2001, 2002, 2003; and Van Biezen and Katz, 2004). The sample contains observations for all 12 countries in the euro area – Austria, Belgium, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, The Netherlands, Portugal and Spain – over the time period 1999–2004. The appendix, which can be found on the EUP webpage, contains a detailed overview of the data sources used.

Results

Columns I and II of Table 2 present benchmark regressions that contain only the economic control variables. The difference between the regressions is the output gap variable used; in column I this variable is constructed on the basis of potential output, whereas in column II it is calculated on the basis of trend income. It follows that only the coefficients for the output gap measures are significantly different from zero. For each regressor, including the constant, an *F*-test was performed to test whether common coefficients could be assumed over all cross-sections in the sample. In all cases, the test failed to reject the null hypothesis of poolability.

In columns III and IV, a pre-election year dummy is added to the model. In both specifications the election coefficients have a negative sign but are not very significant. Moreover, the fit of the model does not improve substantially compared with the benchmark regressions. However, this changes when the pre-election year dummy is replaced by an election year dummy (columns V and VI). Now the adjusted R^2 rises to about .46 and the election effect is highly significant. Also an unexpected increase in GDP growth has a positive and significant effect on the budget balance. However, the coefficient of unexpected inflation remains insignificant. Finally, columns VII and VIII of Table 2 present specifications in which pre-election as well as election year dummies are included. The pre-election year dummy becomes somewhat more significant than before, but the fit of the model decreases slightly compared with the specification in which only an election year dummy is taken into account.

Table 3 presents the results that are obtained when possible election effects are modelled more accurately, that is, by means of the election indicator as suggested by Franzese (2000). In columns I and II an indicator is added with the superscript 'standard', which comprises both pre-election and election years. The coefficients of the output gap and unexpected GDP growth are significantly different from zero. There is also a highly significant election effect. If the election variable is decomposed into separate effects for pre-election and election years, the same pattern as in Table 2 becomes visible. Columns III and IV of Table 3 show that there is no effect in the pre-election year, while columns V and VI indicate the existence of a highly significant effect during the election year. This is underlined by the high adjusted R^2. Columns VII and VIII of Table 3 show that including the two indicators simultaneously does not lead to a significant effect during pre-election years.

Hence, our results suggest that only during the election year does the budget deficit increase. This increase amounts to 0.96% of GDP according to the model with the election year dummy and 0.83% of GDP according to the model with the election year indicator.[6] The latter figure is obtained by

Table 2 Estimation results: Election dummy

Dependent variable	ΔBB							
Equation No.	*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
Constant	.0611	.0306	.1454	.1109	.2818	.2515	.4126	.3820
	(.5072)	(.7422)	(.1568)	(.2871)	(.0002)***	(.0011)***	(.0000)***	(.0001)***
$y - y^f$	.2233	.1813	.2339	.2093	.3420	.3174	.3292	.3162
	(.1503)	(.2474)	(.1247)	(.1744)	(.0044)***	(.0101)**	(.0107)**	(.0190)**
$\pi - \pi^f$	–.0350	–.0743	–.0246	–.0670	–.0332	–.0473	–.0759	–.0917
	(.8070)	(.5894)	(.8816)	(.6864)	(.7547)	(.6383)	(.5882)	(.5202)
$\Delta GAP^{\text{Potential GDP}}$	.2496	–	.2335	–	.1898	–	.1723	–
	(.0702)*		(.1084)		(.0593)*		(.1296)	
$\Delta GAP^{\text{Trend GDP}}$	–	.3055	–	.2740	–	.2180	–	.1871
		(.0203)**		(.0434)**		(.0298)**		(.1018)
$D^{\text{Pre-election year}}$	–	–	–.4928	–.4569	–	–	–.5814	–.5597
			(.0775)*	(.1014)			(.0134)**	(.0184)**
$D^{\text{Election year}}$	–	–	–	–	–.9541	–.9284	–.9726	–.9556
					(.0000)***	(.0000)***	(.0000)***	(.0000)***
Durbin–Watson stat.	2.13	2.17	2.18	2.21	2.04	2.04	2.18	2.18
Adjusted R^2	.20	.22	.25	.25	.46	.47	.45	.44

Notes: Balanced sample of 72 observations and 12 cross-sections; panel generalized least squares (cross-section weights) with common effects; White heteroscedasticity-consistent standard errors and covariance; *p*-values in parentheses: *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

Table 3 Estimation results: Election indicator

Dependent variable	ΔBB							
Equation No.	*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
Constant	.2589	.2154	.0810	.0453	.2346	.2039	.2796	.2409
	(.0069)***	(.0238)**	(.4294)	(.6634)	(.0008)***	(.0042)***	(.0008)***	(.0039)***
$y - y^f$	.2607	.2405	.2202	.1914	.3203	.2935	.3039	.2876
	(.0520)*	(.0734)*	(.1556)	(.2230)	(.0061)***	(.0130)**	(.0134)**	(.0221)**
$\pi - \pi^f$	–.0143	–.0553	–.0345	–.0761	–.0246	–.0501	–.0176	–.0504
	(.9354)	(.7200)	(.8324)	(.6383)	(.8205)	(.6225)	(.8958)	(.7075)
$\Delta GAP^{\text{Potential GDP}}$	.2558	–	.2575	–	.2363	–	.2641	–
	(.0253)**		(.0731)*		(.0215)**		(.0172)**	
$\Delta GAP^{\text{Trend GDP}}$	–	.3063	–	.2973	–	.2603	–	.2724
		(.0094)***		(.0276)***		(.0085)***		(.0105)**
ELE^{Standard}	–1.2285	–1.1732	–	–	–	–	–	–
	(.0007)***	(.0012)**						
$ELE^{\text{Pre-election year}}$	–	–	–.3778	–.3229	–	–	–.6139	–.5648
			(.3745)	(.4352)			(.1023)	(.1262)
$ELE^{\text{Election year}}$	–	–	–	–	–2.2148	–2.2003	–2.2595	–2.2254
					(.0000)***	(.0000)***	(.0000)***	(.0000)***
Durbin–Watson stat.	2.27	2.31	2.16	2.20	2.17	2.19	2.24	2.26
Adjusted R^2	.38	.37	.20	.21	.47	.49	.45	.45

Notes: Balanced sample of 72 observations and 12 cross-sections; panel generalized least squares (cross-section weights) with common effects; White heteroscedasticity-consistent standard errors and covariance; *p*-values in parentheses: *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

multiplying the coefficient estimate for the election year indicator obtained in column VI of Table 3 by the average value of the indicator over all election years (which is 0.38 in our sample).

At first sight it seems rather peculiar that there exists such a large difference between the effects of pre-election and election years. This is especially surprising since the average value for all pre-election year indicators in the sample is 0.65, indicating that in general a large fraction of the 365-day period prior to elections in our sample was part of the pre-election year. However, there is a plausible explanation for this phenomenon that can be derived from the third-generation PBC models as outlined above. In these models, it is assumed that the government uses short-term excess borrowing as a hidden effort – that is, as a policy instrument unobservable to the electorate – in order to increase its performance index. Borrowing extra money during a pre-election year is less easy to hide in comparison with borrowing during an election year because information on the pre-election year's budget deficit is likely to be published prior to the election date. As a result, the policy instrument is no longer unobservable since all excess government borrowing will be noticed by the electorate. Consequently, the increase in the government's performance index will not take place. In fact, the index might even decline if the electorate strongly prefers budgetary discipline.[7]

The mechanism described above also implies that the relationship between the timing of an election during the year and electorally induced fiscal policy manipulations is non-monotonic. There is a trade-off between two effects: a higher value of the election year measure indicates that during that year there is ample time for the government to increase its performance index, but a higher value of the election year variable also indicates that it will be more difficult for the government adequately to hide its efforts from the electorate since more reliable statistics on the size of the budget balance are available to the public.

To test this further, we transform the election year indicator into two dummy variables. The first dummy ($D^{\text{Election in Q1/Q4}}$) is given a value of 1 if an election took place during the first or last quarter of the year; in all other cases its value is set to 0. The second dummy variable is constructed in the same way, but indicates whether an election took place during the second or third quarter. If the hypothesized trade-off indeed exists, the election effect should be strongest for observations covered by the second dummy variable. Columns I and II of Table 4 present the results. Clearly, both models fit the data substantially better than the models estimated before, and the coefficient estimates for both dummy variables suggest that the relationship between the change in net lending and the value of the election year indicator is non-linear. This is further supported by the results from columns III and IV, which

Table 4 Estimation results: Election year indicator with non-linear effect

Dependent variable	ΔBB					
Equation No.	*I*	*II*	*III*	*IV*	*V*	*VI*
Constant	.2409	.2223	.2565	.2321	–.4433	–.4102
	(.0000)***	(.0000)***	(.0000)***	(.0001)***	(.0166)**	(.0278)**
$y - y^f$	.3761	.3634	.3621	.3444	.3116	.3108
	(.0000)***	(.0000)***	(.0002)***	(.0006)***	(.0005)***	(.0011)***
$\pi - \pi^f$	–.0123	–.0351	.0084	–.0129	–.0354	–.0750
	(.8882)	(.6841)	(.9296)	(.8867)	(.6739)	(.3887)
$\Delta GAP^{\text{Potential GDP}}$	.1846	–	.2009	–	.2981	–
	(.0069)***		(.0138)**		(.0002)***	
$\Delta GAP^{\text{Trend GDP}}$	–	.1876	–	.2140	–	.2785
		(.0056)***		(.0076)***		(.0003)***
$D^{\text{Election in Q1/Q4}}$	–.3725	–.3803	–	–	–	–
	(.1306)	(.0933)*				
$D^{\text{Election in Q2/Q3}}$	–1.2891	–1.2621	–	–	–	–
	(.0000)***	(.0000)***				
$ELE^{\text{Election year}}$	–	–	–5.4486	–5.2460	–5.5918	–5.2627
			(.0000)***	(.0000)***	(.0000)***	(.0000)***
$(ELE^{\text{Election year}})^2$	–	–	6.0190	5.7320	6.4389	5.9783
			(.0001)***	(.0001)***	(.0000)***	(.0000)***
GCOL	–	–	–	–	.0638	.0582
			–	–	(.0002)***	(.0007)***
Durbin–Watson stat.	2.16	2.16	2.10	2.10	2.12	2.13
Adjusted R^2	.70	.68	.61	.61	.69	.67

Notes: Balanced sample of 72 observations and 12 cross-sections; panel generalized least squares (cross-section weights) with common effects; White heteroscedasticity-consistent standard errors and covariance; *p*-values in parentheses: *, ** and *** denote significance at 10%, 5% and 1% level, respectively.

account for a non-linear election effect by using a squared election year indicator in addition to the original one. The model fit is quite good, and the coefficients for the election year variables are highly significant.[8] The pattern that can be derived from the coefficient estimates for the election effect (based on column III of Table 4) is depicted in Figure 1. The graph shows that fiscal policy becomes more expansionary when the election date is closer to the middle of the year.

Sensitivity analysis

In order to test the robustness of our results with respect to sample composition, we estimated equations III and IV of Table 4 12 times, each time excluding one country from the sample. Although the exclusion of France results in an insignificant coefficient for the unexpected growth variable,[9] the election effect is always highly significant (results available on request).

In addition, we estimated specifications III and IV using alternative indicators of fiscal policy that have been suggested in the literature as the dependent variable. First, while excluding the output gap as a regressor, we used

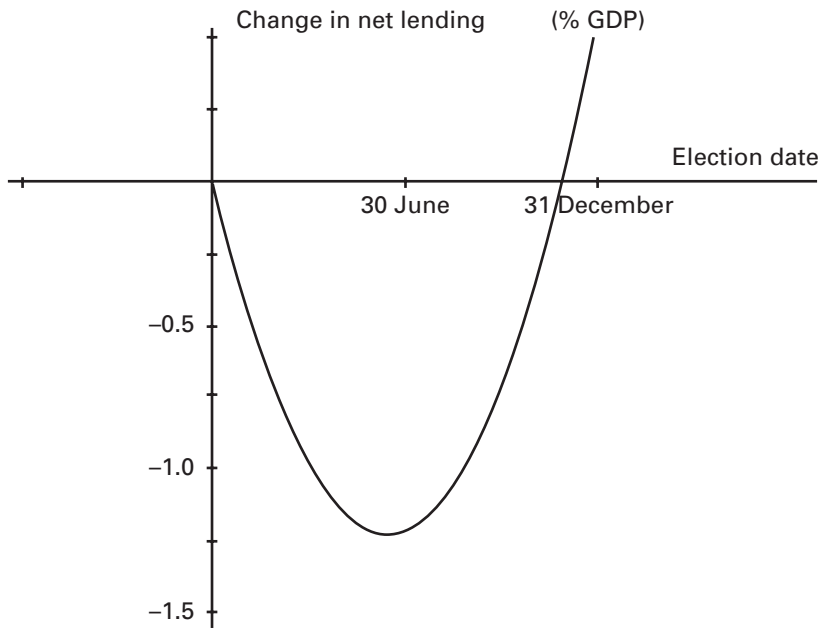

Figure 1 Graphical representation of the election year effect.

cyclically adjusted changes in net lending. Second, we employed the level of net lending while adopting the output gap level instead of the change therein as a regressor. And, third, we used changes in the primary balance. In all cases, the outcomes were highly comparable to the ones originally obtained, the election year effect being slightly weaker in the first case, somewhat stronger in the second, and virtually unchanged in the third (results are available on request).

Next, we examined whether our findings are influenced by the inclusion of variables reflecting political-institutional conditions that have been suggested to affect fiscal policy. Therefore, in specification III of Table 4 we included variables reflecting (i) the type of government, (ii) the fractionalization of government, and (iii) the positioning of the government on a Left–Right scale.[10]

Since Roubini and Sachs (1989), various studies have examined the impact of the type of government on fiscal policy outcomes. The basic idea is that coalition governments are weaker than single-party majoritarian governments and therefore will end up with higher deficits if the country concerned is hit by a negative economic shock. Roubini and Sachs (1989) present evidence in support of this view, but other studies reject the hypothesis (see, for example, De Haan and Sturm, 1997). To take the type of government into account, we constructed a dummy, which is 1 in the case of a single-party government and 0 in all other cases. It turned out that the dummy's coefficient was positive but highly insignificant, with a *p*-value of .54. The other results remained unchanged.

Perotti and Kontopoulos (2002) argue that the fractionalization of government – measured by the number of political parties or the number of spending ministers in government – affects fiscal policy. The more fragmented the government, i.e. the greater the number of parties or spending ministers, the higher will be the budget deficit. There is quite a lot of support for this view (see, for instance, Volkerink and De Haan, 2001). Therefore, the number of political parties in government and the number of spending ministers in government were separately included in the regressions. They both failed to reach significance (*p*-values of .18 and .91, respectively) and did not affect the election effect.

As Franzese (2000) puts it, at least since Hibbs (1977) political economists have argued that parties of the Right and Left differ in their fiscal policy priorities. Specifically, left-wing governments favour larger public economies, greater redistribution and more Keynesian expansion, and so are expected to run greater deficits than right-wing governments. Since several empirical studies indeed suggest that the political colour of a government affects fiscal policy outcomes (see Franzese, 2002, for a discussion), the influence of the

government's positioning on a Left–Right scale was also taken into account. This was done by constructing the variable *GCOL*, which uses a scaling from 1 for ultra-left to 20 for ultra-right governments. Data on party colour were obtained from Benoit and Laver (2005). The estimation results are presented in columns V and VI of Table 4. The political composition of the government does indeed have a significant influence on the change in net lending. However, this influence is rather small. Again, adding this variable does not substantially affect the coefficient of our election variables.

Finally, some specific non-political variables were added to the model as a further robustness test. A number of previous studies have suggested that there may be asymmetries in fiscal responses to recessions and upturns. For instance, Mayes and Viren (2000) argue that during recessions governments pursue expansionary fiscal policies to combat the downturn, whereas during economic upswings the proceeds are used for cutting taxes or increasing spending rather than for additional consolidation. Therefore, a dummy is constructed that is 1 in the case of a positive change in the output gap and 0 otherwise. Because it is highly insignificant, with a *p*-value of .77, there is no reason to assume that fiscal policy responds differently in magnitude to positive changes in the output gap than to negative changes. Including the variable did not affect our other results.

The debt-to-GDP ratio was also added to the model because it may capture concerns about the sustainability of fiscal policy (Tujula and Wolswijk, 2004). From a sustainability perspective, increasing debt ratios should lead to an improvement in the budget balance. In fact, Melitz (2000) suggests the use of a quadratic formulation for the debt ratio because higher debt levels may induce more stabilizing fiscal reactions. However, a higher debt ratio also causes a rise in interest payments, resulting in a worsening fiscal balance. As a consequence, the overall effect of the debt ratio on budget balances is not a priori clear. Indeed, the coefficients of the level and the squared level of the debt-to-GDP ratio are insignificant, with *p*-values between .15 and .22. Again, the election effect remains unchanged.

Conclusions

As Buti and Van den Noord (2003) point out, in the run-up to the monetary union the incentives for national policy-makers to stick to the announced consolidation path were quite strong because failure to meet the convergence criteria carried the penalty of exclusion from the euro area. This was considered too harsh a political sanction. However, things may be different

once a country has been admitted to the monetary union. That is, unlike the Maastricht convergence criteria, sticking to the rules of the SGP may not pay politically. This holds true especially for large countries (see De Haan et al., 2004). Moreover, the adoption of fiscal consolidation policies during the run-up to the monetary union might have actually created the room for manoeuvre for politically motivated fiscal actions.

This article has examined whether there is a PBC in EU member countries in the euro area despite the adoption of the Stability and Growth Pact. We have concentrated on EU members that have adopted the euro because the rules of the SGP are less strict for member countries that are not part of the currency union. We use a more sophisticated approach than previous studies that focused on the recent fiscal policy experience of EU member countries. Using data for the period 1999–2004, we have found strong evidence that the SGP did not withold policy-makers from pursuing expansionary fiscal policies before elections. In an election year – but not in the year prior to the election – the budget deficit increases. This result is in line with third-generation PBC models, which are based on moral hazard. We have also concluded that fiscal policy is somewhat more expansionary under left-wing policy-makers.

Finally, two important caveats are in order. First, since the currency union started only six years ago, our analysis is based on a short sample period. Nonetheless, given the relatively large number of elections in the period 1999–2004 – all countries had general elections as part of the regular electoral cycle – our evidence clearly indicates the existence of electorally motivated fiscal policies. Second, our analysis is based on aggregate measures of discretionary fiscal policy and may therefore not fully capture PBCs. Governments may lower taxes or give extra subsidies to special interest groups in order to maximize the probability of being re-elected, and such changes may not show up in aggregated data. A suggestion for future research is therefore to use more disaggregated measures of discretionary fiscal policies.

Notes

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1 A political budget cycle is a periodic fluctuation in a government's fiscal policies, which is induced by the cyclicality of elections. Shi and Svensson (2004) review some recent developments in the theory and evidence of these cycles. See also Franzese (2002) and Drazen (2001) for extensive reviews of the literature.

2 There are exceptions, however. For instance, Alesina et al. (1997), who perform cross-section time series regressions on a panel of 13 OECD countries for the period 1961–93, find that the government budget deficit is 0.6% of GDP higher in election years. Also Franzese (2000) reports evidence for electoral manipulation of fiscal policy in his sample of 20 OECD countries.
3 However, the evidence of Von Hagen is not very convincing. For instance, he takes both presidential and parliamentary elections into account even though presidents in some countries have hardly any influence on policy-making. De Haan et al. (2003) could not replicate his findings.
4 See Amtenbrink and De Haan (2003) for an extensive discussion of the SGP. De Haan et al. (2004) examine the weaknesses of the SGP. See also Schuknecht (2004).
5 The first part of this section heavily draws on Andrikopoulos et al. (2004) and Shi and Svensson (2004). See Lohmann (1998) for an analysis of monetary policy.
6 This conclusion is not affected by the exclusion of snap elections in calculating the election indicators (results available on request).
7 There is some evidence (see, for instance, Brender, 2003) suggesting that a deterioration in the financial position of the government harms the incumbent's chances of re-election. Brender (2003) argues that this should be the case in all 'well developed democracies' where voters can monitor the evolution of public finances. Drazen and Eslava (2003) try to reconcile this fiscal conservativeness and the PBC.
8 A caveat is in order here since the sample contains only three observations for which the election year indicator has a value greater than .5. However, the squared election indicator becomes insignificant only when all three of these observations are removed, making it unlikely that it is based on an outlier. Hence, even though all other results also suggest a non-linear election effect, further research into the non-linear effect is warranted once more data are available.
9 This could be the result of collinearity between forecasting errors in growth rates and changes in the output gap since the correlation between both variables is .75. However, this does not affect our main conclusions because both regressors are used only as control variables meant to pick up the impact of non-discretion fiscal policy on the budget balance.
10 We did not include an indicator of budgetary institutions because figures are not available for all countries over our estimation period. See, for instance, Hallerberg (2004) for a discussion.

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